

maximum of the year 1790 to that of 1850, was thence concluded, perhaps too confidently, as the real length of this long cycle of auroral frequency.

On turning to Kæmtz's "Meteorology" (translation by C. V. Walker, 1848, p. 458), I find that the author, with his usual exhaustive completeness, has constructed a general list of auroras observed up to his time (about the year 1820), and has established from it certain laws of their periodicity. The list itself, although not given for brevity in the translation, is in all probability contained in the original, and it must embrace upwards of 3,000 cases of auroral occurrences, since a table of about that total sum showing the numbers recorded in each of the several months of the year is given as the most important scientific result of the compilation. The numbers seen in March, September, and October are about half as great again as those recorded in any of the adjacent months, and about twice as great as have been recorded in either of the two mid-winter months of December and January, when the length of the nights is yet most favourable for their registry. That the numbers of auroral displays noted in June and July are relatively very small is easily explained by the length of the twilight in those months in European latitudes, rendering many, that would be conspicuous exhibitions in darker nights, invisible.

The times of greatest annual activity of the auroræ are thus about the seasons of the equinoxes, when the seat of the most direct action of the sun's rays upon the earth's surface is undergoing its most rapid changes during the sun's yearly course; and when nearly the same parts of the earth's surface continue to be heated directly by the sun's rays at the seasons of the winter and summer solstices, there are times of comparative repose and tranquillity among the exhibitions of auroral outbreaks.

Regarding a secular period, Kæmtz's Catalogue appears to have shown nothing positive. "A period of this kind," he writes, "occurred between the years 1707 and 1790, attaining its maximum about the year 1752; since the year 1820 they have again continued to become more numerous." This maximum in the year 1752, and those shown on Prof. Loomis' auroral curve about the years 1780-90, 1850, and 1870-72, agree very ill with each other, or with the return of a constant cycle of long period connecting them together; the succession more nearly resembles that of periods of hot summers, or of cold winters, governed by fixed laws that have not yet been discovered in their returns and durations; and seems to point to causes influencing the production of auroras very similar to those which determine some of the obscurest features of our seasons. Thus, since the commencement of the earliest continuous temperature-records at the Royal Observatory, Greenwich, in the year 1771, the commencement of winter or the arrival of a mean daily temperature of 40° has fluctuated between the months of November and December, apparently from different degrees of prevalence in those months of an annual tide of south-west wind then reaching a maximum in the British Isles. Assuming changes in the strength of this wind to be the cause of the observed fluctuations and of a gradually increasing retardation of winter and secular rise of mean temperature in the months of November, December, and January, noticed by Mr. Glaisher during the first half of the present century, the average course of this phenomenon, when submitted to examination, resembles very closely the general course of the curve of auroral frequency. There was a sensible retardation of the winter season from about the year 1775 to about the year 1790, followed by a marked acceleration from the latter year onwards through nearly the first quarter of the present century, indicating apparently a considerable abatement of south-west, anti-trade, or equatorial currents, on an average, for that lengthened period. The acting cause however returned, and its strength may be gathered from the fact that the mean temperature of the month of December at Greenwich during the twenty-five years from 1825 to 1850 was higher in eight years than that of the month of November, an anomaly which had only taken place thrice in the first quarter of the century. The last occurrences of the same kind, with which I am acquainted, happened in the years 1858, 1861, and 1862; but the strong retardations of winter, noticeable towards the year 1850, were then rapidly disappearing, and it is not improbable that in the further fluctuations that have since followed, a new correspondence between the secular rise of temperature of the months of November, December, and January at Greenwich, and the considerable maximum of auroral intensity reached during the years 1870-1873, may be found to bear out an analogy which is only hazarded here, in the absence of a better working hypothesis, as an apparently real and perhaps not altogether unnatural connection.

With regard to the relative proportion between eastward and westward movements of auroral rays, I know of no observations that have been made that can offer Mr. Procter any additional information. The possibility that auroral streamers may be uprushes of positive or negative electricity to a point of saturation in the highest regions of the atmosphere, followed by downrushes of the same electricity when the exciting cause in the interior or on the surface of the globe subsides, might be well proved by such observations. The existence of the motion shows that the auroral rays diverge sensibly from the earth's lines of magnetic force, probably in the endeavour (whether effectual or not is indifferent to the explanation) of the Aurora Borealis and Aurora Australis to combine and to neutralise each other (perhaps a rare occurrence) across the equator. The strength of the motion of the beams may be some measure of this tendency, and its absence a sign that the aurora is local and of comparatively little generality and extent. It may here be remarked that the annual periodicity of auroras differs entirely from that observed in the average frequency of sporadic shooting stars, which reaches a maximum in August and September, but has a well-marked minimum in March, resembling the single cold of winter and the single heat of summer produced three months earlier, in each year, by the tropical motion of the sun. A marked frequency of auroras on the dates of January 1-3, April 19-21, August 9-11, October 18-21, November 14 and 27, and December 10-12, when meteor-showers of various degrees of brightness are of almost annual occurrence, has not, as far as I am aware, been definitely traced and established; but the large auroral catalogues recently published by Prof. Loomis and Prof. Lovering will, it is evident, supply very valuable materials by which any such connection between auroras and periodical meteor-showers, if it exists, can be more thoroughly investigated and determined.

A. S. HERSCHEL

Automatism of Animals

YOUR correspondent, Mr. Wetterhan, has, I think, misunderstood Prof. Huxley's argument; which is, not that the adjusted motions he refers to never were the result of conscious and voluntary motion, but that they are not so now. His letter has, however, induced me to call attention to what has always seemed to me a real difficulty. As I understand automatic or reflex actions, they are those which have been so constantly repeated and which are so essential to the well-being of the individual, that the various nerves implicated have become so perfectly co-ordinated that the appropriate stimulus sets the whole machinery in motion without any conscious or voluntary action on the part of the individual. Thus we can quite understand how a paralysed limb would be drawn up when the sole of the foot is tickled or the toe pricked. If, however, any such irritation continues to be felt in the normal state, a man would stoop down and remove the irritating substance with his hand, or would place his foot upon the opposite knee, and, stooping down, endeavour to see the object which caused the irritation. But these are *conscious*, not *reflex*, acts. They are not repeated often enough, and are not sufficiently identical in form, to become automatic; and we are not told that a wholly paralysed human body does actually go through these various motions, as it certainly would do if not paralysed.

Now, in the case of the frog I can quite understand the jumping, swallowing, swimming, and even the balancing; for all these are actions so essential to the animal's existence, and so often repeated during life, as to have become automatic. So, also, I can understand the drawing up of the foot to remove an irritation on the side of the body, for with the short-necked frog this too is an essential, and must have been an oft-repeated action. But we are further told that "if you hold down the limb so that the frog cannot use it, he will, by and by, take the limb of the other side and turn it across the body, and use it for the same rubbing process." Now, this seems to me not to be explicable by automatic or reflex action, because it cannot have been an action frequently if ever performed during the life of every frog. It is true that from the co-ordination of the movements of the opposite limbs, we might expect, if the irritation were continued, and the leg on the same side kept for some time in motion, that the other leg would begin to move in the same way. But what causes it to move in a quite different and unusual way, *across* the body to the opposite side; and this, as related, at once and without first trying its own side? The most usual motion of both legs is directly up and down, each on its own side. What is it that causes one of these legs, when it

begins to move, not to move in the usual way (that which is automatic during life), but in an unusual manner, which must have been very rarely, if at all, used during life, and when used must have been purely conscious and voluntary? I think I cannot be mistaken in considering this to require some explanation. It may be that the frog is constantly, during life, crossing one foot over to rub the opposite side of the body; but we cannot accept this as an explanation unless it has been observed to be a fact. What puzzles me is, that Prof. Huxley, Dr. Carpenter, and Mr. Darwin, all refer to this case as an example of reflex action, and none of them see any difficulty in it, or seem to think that it requires any more explanation than the remaining quite intelligible cases. As others may, like myself, feel the difficulty I have endeavoured to point out, I hope some of your physiological correspondents will enlighten us if they can.

ALFRED R. WALLACE

Supernumerary Rainbow

IN Mr. Backhouse's letter (*NATURE*, vol. x. p. 437) he remarks that the supernumerary rainbow is commonly seen only in the upper part of the arch. Dr. Thomas Young, in his Bakerian Lecture ("Works," vol. i. p. 185, or *Phil. Trans.* 1804), after explaining the supernumerary bow by interferences, quotes a paper in vol. xxxii. of the *Phil. Trans.*, in which Dr. Langwith describes his observation of a supernumerary bow on August 21, 1722; then remarks: "I have never observed these inner orders of colours in the lower parts of the rainbow. I have taken notice of this so often that I can hardly look upon it as accidental; and if it should prove true in general, it will bring the disquisition into a narrow compass; for it will show that this effect depends upon some property which the drops retain whilst they are in the upper part of the air, but lose as they come lower and are more mixed with one another." But I am not aware that anyone has ever remarked an appearance which struck me on seeing a few days ago a very complete primary and secondary bow with a portion of two supernumerary bows within the primary and about the highest part of the arch. To my eye the supernumerary bows were *not concentric* with the primary. My son agreed with me as to this appearance when I pointed it out to him; yet I thought it was probably an illusion till the following explanation occurred to me.

The rain-drops may be presumed to be smaller high in the air, and to increase as they descend.

Now, the smaller drops produce wider interference fringes than the larger drops do. Hence the supernumerary bow is widest and therefore farthest from the primary at the top of the arch, and gets narrower and nearer to the primary as it descends the arch on each side, and "in the lower parts" ultimately fines away to nothing. According to this theory the supernumerary bow is not always concentric with the primary, nor indeed circular.

It should be observed that another reason for the interference bow being seen most frequently at the highest part of the bow is that the small drops high in the air are probably more uniform in size than the larger drops lower down.

Oct. 8

JOSEPH BLACKBURN

Colour in Flowers not due to Insects

THE doctrine that the conspicuous colours of flowers are entirely due to the necessity for cross-fertilisation by the agency of insects seems to be taking the world by storm. It is supported by Mr. Darwin and Sir John Lubbock. It could scarcely be put forward on better authority. Yet there are several facts with which it does not harmonise. For instance—

1. *Cultivation* increases the size and colour of flowers quite independently of the existence or non-existence of insects.

2. *Double flowers* in which the doubling arises from metamorphosis of stamens or pistils are more showy than the single forms, yet insects can be of little use to them, since they are either partially or entirely barren. The double-blossomed cherry is brilliantly conspicuous, but it bears no fruit.

3. Such *abortive flowers* as the cultivated Guelder Rose and Hydrangea depend for their beauty upon the destruction of the reproductive organs. If their increased splendour is meant only as a lure to insects, it is surely an absurd failure.

4. The *autumn colours* of leaves and fruits can serve no such purpose, yet these are often as bright and conspicuous as the flowers of summer.

5. *Fungi and lichens* exhibit brilliant colours, which can have nothing to do with insect-fertilisation.

Do not these facts indicate that though insects may be attracted by conspicuous colours, and may have some influence in the maintenance of coloured species, there is yet a deeper and more permanent cause for the colour itself?

Leicester, Oct. 11.

F. T. MOTT

Habits of Squirrels

WOULD you permit me to ask of your readers a question or two upon the habits of squirrels? I have had one in my possession, from the age of three weeks, for more than two years. I have noticed that whenever it cleans itself, after licking, it *sneezes* violently three or four times into its forepaws, then rubs them thus damped over its fur. It seems to have the power of sneezing at volition.

Now, is this habit of sneezing, for the purpose of cleaning itself, a habit peculiar to squirrels; or is it shared by other animals?

I notice also that frequently when it is going thoroughly to clean itself it jerks its forepaws over its ears, bringing them back over its eyes, and always causing a milky liquid to suffuse the eyes. This liquid swims over the eye, and then is absorbed. I have thought that it may use this secretion also for the purpose of moisture. The animal is in perfect health and splendid condition.

A squirrel I had three years ago also had this habit, though in a slighter degree.

D. T.

THE NEW VINE-DISEASE IN THE SOUTH-EAST OF FRANCE

I.

WE have before us the Reports presented to the French Academy of Sciences by the delegates of the Commission appointed by that body to investigate the phenomena of the new and terrible disease of the vine in the south-east of France—a disease which is fraught with the most serious consequences to the material prosperity of that country, which depends on its wine as a source of national wealth not less important than are our coal and iron to us.

It was in the autumn of the year 1871 that the Academy of Sciences directed special attention to the communications which poured in upon it from all quarters relative to the ravages of the new parasite of the vine in the South of France; and at the sitting on the 25th September in that year, it charged a Commission, consisting of M. Dumas as president, MM. Milne-Edwards, Duclaux, and Blanchard, to investigate the means of coping with the disease. The Commission examined with the greatest care all the manuscripts and printed monographs which were brought under its notice, and paid particular attention to the scrutiny of the leaves and the roots attacked by the *Phylloxera vastatrix* (for such is the name which has been given to the new insect), which had been sent to it from different places in France; and, with the object of giving to its labours the active direction necessary in such circumstances, it decided to confide the execution of them to three delegates, viz. MM. Balbiani, Max Cornu, and Duclaux, whose learned researches in zoology, botany, and chemistry, suggested recourse to them, and they were accordingly charged with the pursuit of all the observations which the subject would allow of, on the actually affected territory.

It is worth our while, at the outset, to observe the thorough and methodical manner in which an attempt has been made to wrestle with this new enemy of the material welfare of France, and the application of the resources of science to unravel as exhaustively as possible the causes and manner of extension of the invasion of the parasite from its first appearance till the present time. We in England are too apt in similar crises to neglect the practical employment of scientific means, to depend on private and individual exertions for the investigation and treatment of the different causes which threaten the national wealth or